tinuous current beyond that region cannot be proved by observations, either of temperature or movement.

IX. That the Gulf-stream and other local currents put in motion by the trade-winds or other influences acting on the *surface* only, will have as *their* complement in a *horizontal* circulation return *surface* currents; and that the horizontal circulation of which the Atlantic Equatorial Current and the Gulf-stream constitute the first part is completed—so far as the Northern Hemisphere is concerned—partly by the direct return of one large section of the Gulf-stream into the Equatorial Current, and as to the other section, by the *superficial* polar currents which make their way southwards, the principal of them even reaching the commencement of the Gulf-stream.

In conclusion it may be added that the doctrine of a general vertical oceanic circulation is in remarkable accordance with the fact now placed beyond doubt by the concurrent evidence of a great number of observations, that whilst the density of oceanic water, which is lowest in the Polar area, progressively increases as we approach the Tropics, it again shows a decided reduction in the intertropical area. It has been thought that an explanation of this fact is to be found in the large amount of rainfall and of inflow of fresh water from great rivers in the intertropical region; but it is to be remembered that the surface evaporation also is there the most excessive, so that some more satisfactory account of the fact seems requisite. Such an explanation is afforded by the doctrine here advocated; the Polar water which flows towards the Equator along the bottom of the ocean basins, being there pumped up and brought to the surface.* And it is not a little confirmatory of the views advanced in this Report that in a recent elaborate discussion of the facts relating to the comparative density of oceanic water on different parts of the earth's surface, the doctrine of a general vertical circulation is advocated as affording the only feasible rationale of them.†.

SCIENTIFIC SERIALS

THE *Zeitschrift fur Ethnologie* (1870 Heft III. and IV contains the following notices:—Orton's "Andes and the Amazon."—Waring's "Stone Monuments, Tumuli, &c.," "Manuscrit Troano," giving an account of the MS. in question, which is written in the Maya language; the reviewer calls this "surely the wildest production that ever saw the light with the sanction of an Imperial Government," though he admits that still wilder productions are published in his own country, now also under "Imperial government."—Benfey's "Gesch. de Sprachwissenschaft" is highly praised.—Burgen's "Temples of Satrunjaya," with forty-five photographs.—Hamy's "Paléontologie Humaine."

THE last part of the "Neues Jahrbuch fur Mineralogie, Geologie," &c., published 1871, contains the following papers:—R. D. M. Verbeek on the Nummulites of the Borneo Rocks, with three plates illustrating new species, &c., one species, *N. Biaritzensis*, is also found in these beds, and extends through all the nummulitic formation from the Pyrenees to Borneo. He believes that this formation extends to Java and most of the islands of the East Indian Archipelago, but hitherto this formation has not been recognised.‡—Dr. R. Lincke on the Buntersandstein in Thüringen, which is the commencement of an elaborate monograph on these beds.—Dr. Allred Stelzner on Quartz and Allied Minerals.—Adolph Pichler, Additions to the Mineralogy of the Tyrol; and, by the same author, Additions to the Palæontology of the Tyrol, and the usual mineralogical, geological, and palæontological notices.

OF the Transactions of the Natural History Society of Rhenish Prussia and Westphalia, including also the reports of the Society of Natural History and Medicine of the Lower Rhine, we have received the twenty-sixth volume, containing an account of the doings of the respective societies in the year 1869. The papers published by the first-mentioned society are well known to naturalists, and often of very great value. In the present volume we find the following:—"Contributions to the Rhenish Flora," by Dr. P. Wirtzen, including a discussion of the species of dog-roses, with the description of a so-called new species, *Rosa exilis*, a notice of *Asplenium Heufleri*, the description of a new plantain from Saarbruck, *Plantago Winteri*, a notice of the various forms of *Rubus tomentosus*, and of anomalies in other species of *Rubus*, and notices on the geographical distribution of certain plants; also, by the same author, a supplement to his manual of "The Flora of Rhenish Prussia; a paper "On the Height of the Water of the Rhine at Cologne from 1811-1867," by M. H. von Dechen; the continuation of Kaltenbach's valuable memoir on the German Phytophagous insects, in which the species feeding upon each species of plant are noticed, the plants being arranged in the alphabetical order of their botanical names, now reaching to the end of the letter S.; a contribution to the knowledge of the cryptogamous flora of the Saar district, by M. F. Winter, containing notices of Equisetaceæ, Lycopodiaceæ, and Ferns; and a paper, illustrated with three plates, "On the Fossil Echinodermata of North Germany," by Dr. C. Schlüter. In the last-mentioned paper, the author notices the described species of Jurassic and Cretaceous Echinoderms found in North Germany, and describes several new forms. The reports of the second society mentioned, which holds its meetings in Bonn, include an immense number of short notices of communications on almost all branches of science, but especially on Natural History and Chemistry; many of them are of considerable interest.

IN the March number of the *Journal of Anthropology* there is a careful anatomical description of the body of a negro by Dr. Koperméki. Detailed measurements are added, together with the weights of the principal organs, and the diameter of more than twenty of the nerves. A remarkable feature in the case was the state of atrophy in which the supra-renal bodies were found; and if, in the absence of other fatal lesions, this may be assumed as the cause of death, there is here recorded a case of Addison's disease occurring in a negro. In the same journal is a translation of a review by Rütimeyer of Prof. Bischoff's work on the skulls of the anthropoid apes, in which both the text and the atlas of plates which accompanies it are severely criticised. Both the original pamphlet and the review have, however, lost much of the interest they possessed at the times of the publication, 1864 and 1868 respectively.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, March 30.—"Contributions to the History of Orcin.—No. 1. Nitro-substitution Compounds of the Orcins." By John Stenhouse, LL.D., F.R.S. The action of nitric acid upon orcin has been studied by several chemists, but with comparatively negative results. Schunck in this manner obtained a red resinous substance, which, by further treatment with the acid, was oxidised with oxalic acid; and in 1864 De Luynes found that orcin dissolved in cooled fuming nitric acid without evolution of nitrous fumes, and that the addition of water precipitated a red colouring matter; the long-continued action of the vapour of fuming nitric acid on powdered orcin likewise produced a red dye apparently identical with the above. These, however, are resinous uncrystallisable substances. Although under ordinary circumstances only resinous products are obtained by treating orcin with nitric acid, yet, when colourless orcin in fine powder is gradually added to strong nitric acid, cooled by a freezing mixture, it dissolves with a pale brown coloration, but without the slightest evolution of nitrous fumes. If this solution be now slowly dropped into concentrated sulphuric acid, cooled to -10° C., the mixture becomes yellow and pasty, from the formation of nitro-orcin, which is but slightly soluble in sulphuric acid. When this is poured into a considerable quantity of cold water, the nitro-body separates as a bright yellow crystalline powder, quite free from any admixture of resin. The crude nitro-orcin was collected, washed with a little cold water, and purified by one or two crystallisations from boiling water (40 parts). It was thus obtained in large yellow needles, which are readily soluble in hot water and but slightly in the cold; the addition of a strong acid precipitates almost the whole of the nitro-orcin from its cold aqueous solution. It is soluble in alcohol, very soluble in hot benzol, and crystallises out in great part on cooling; it is less soluble in ether, and but moderately so in bisulphide of carbon. It dyes the skin yellow, like picric acid, but is tasteless. It volatilises slightly at 100° C., melts at 162° C., and decomposes with slight explosion imme-

* That water of a *lower* should thus underlie water of a *higher* degree of salinity in travelling from the Pole to the Equator, is not difficult to account for, when the relative temperatures of the two strata are borne in mind.

† Densité, Salière, et Courants de l'Océan Atlantique, par Lieut. B. Savy, Annales Hydrographiques, 1868, p. 620.

‡ It is not out of place to mention here that Baron Reichtofen has quite recently found this nummulitic formation in China; it is described in *Silliman's Journal* for February 1871. It has been found also in Japan.

diately afterwards. When heated with concentrated sulphuric acid it dissolves, forming a deep yellow solution, which deposits crystals on cooling, and is immediately precipitated by water. It dissolves in hot strong nitric acid with evolution of nitrous fumes and formation of oxalic acid. Like picric acid, when treated with calcium hypochlorite it yields chloropicrin at the ordinary temperature. Its aqueous solutions are coloured dark brown by ferric chloride, and completely precipitated by lead subacetate. The analysis of the substance dried at 100° C. was made with the following results derived from three experiments:—

	Theory	I.	III.	II.	Mean.
C_7 =	84 = 32·43	32·58	32·63	32·68	32·63
H_5 =	5 = 1·93	2·06	2·18	2·03	2·09
N_3 =	42 = 16·22	..	..	..	..
O_8 =	128 = 49·42	..	..	..	..
	259 100·00				

These results correspond to the formula $C_7 H_5 (NO_2)_3 O_2$ that of trinitro-orcin. It is a powerful acid, much resembling picric acid, but distinguished from the latter by the greater solubility of its salts. I propose, therefore, to call this new substance *trinitro-orcinic acid*. The preparation and composition of a large number of compounds of the acid were then detailed.

Geological Society, March 22.—Prof. John Morris, Vice-President, in the chair. Messrs. A. R. Selwyn, Director of the Geological Survey of Canada; J. Bridges Lee, the Rev. Thomas Robert Willacy, B.A., and James Putnam Kimball, Ph.D, New York, were elected Fellows of the Society. The following communications were read: 1. "On the Passage Beds in the neighbourhood of Woolhope, Herefordshire, and on the discovery of a new species of *Eurypterus*, and some new Land-plants in them." By the Rev. P. B. Brodie, M.A., F.G.S. The author described as the "passage-beds" between the Silurian and Old Red Sandstone formations near Woolhope, a series of shales and sandstones, which at Perton attain a thickness of about 17 feet. Here the section includes, in descending order:—(1) Thin-bedded sandstones; (2) Dark brownish shales; (3) Yellow sandstone; (4) Olive shales; (5) Thin bedded-sandstone; (6) Olive shales, similar to No. 4. At some localities vegetable remains (*Lycopodites*, and perhaps *Psilophyton*) occur in the olive shales, which also contain several Crustacean fossils, including *Pterygotus Banksii* and a new species of *Eurypterus*, named by Mr. Woodward *E. Brodiei*. Upon this species, Mr. Woodward presented a note supplementary to Mr. Brodie's paper. Mr. Duncan inquired whether any metamorphoses had been recognised among the Eurypteridæ, and, if so, whether the variation in the thoracic plates mentioned by Mr. Woodward might be connected with them. Mr. Woodward, in reply, remarked on the difficulty of distinguishing even the sexes in Eurypteridæ. The thoracic plate in the fossils resembled that of *Limulus*, and the variety might be connected with sex. In some *Slimoniæ* from Lesmahago the only difference to be found was in the thoracic plate, and it had been suggested that this was due to difference of sex. He had already suggested that the small *Pterygotus* and the great *Slimonia* might be only the male and female forms of the same species. On fragmentary remains it was, however, unsafe to attempt to base species; but he thought *Eurypterus Brodiei* was a well-marked species. Rev. E. Winwood inquired whether there was any evidence as to *Euypterus* being freshwater or marine. The chairman observed that the seeds from the passage-beds did not appear to him other than those of land-plants, and had been previously described by Dr. Hooker as spore-cases of Lycopodiaceæ.—2. On the Cliff-sections of the Tertiary Beds west of Dieppe in Normandy and at Newhaven in Sussex." By Mr. William Whitaker, F.G.S. The author gave details of the sections of the Tertiary beds at the above places, and noticed the occurrence of London clay. Below this formation at Dieppe is a mass of sand, the same as that of the "Oldhaven beds" in East Kent, but here less markedly divided from the clay above; and beneath this sand come the estuarine shelly clays, &c., of the Woolwich beds. In the older accounts of the Newhaven section a much less thickness of the Tertiary beds is chronicled than may now be seen; indeed the successive descriptions end upwards with higher and higher beds, owing to the destruction of the coast and the wearing back of the cliff into higher ground, the highest point seeming to have been at last reached. Here the Oldhaven sand is absent, but the Woolwich clays are in greater force; and the ditch of the new fort shows some very irregular masses of gravel more or less wedged into those clays. Both sections show the comparatively wide extent of like conditions to those of the Woolwich beds, of West Kent. The Chairman, in inviting discussion, called attention to the existence of Tertiary beds of similar character near Epernay and Rheims, and in other parts of France. Mr. Evans remarked on the bearing which this extension of soft, yielding strata had on the excavation of the Channel. The disturbances in the sands and clays might be due to the springs having formerly, owing to the distance of the sea and the river-valley not having been excavated, stood at a higher level, and having thus softened or even washed away, the bed beneath the gravels. Mr. Pattison mentioned that in all the combes along the French coast towards Tréport there were traces of soft Tertiary beds, possibly Thanet sands. Mr. Whitaker, in a reply to a qu estion from the Chairman, stated that, to the best of his belief, the sandstones at Dieppe were not calciferous. The sands were above the Woolwich beds, and therefore not Thanet sands.—3. "On New Tree Ferns and other Fossils from the Devonian." By Prof. J. W. Dawson, L.L.D., F.R.S., F.G.S. The author referred to the numerous species of ferns known in the Upper and Middle Devonian of America, and to the fact that he had described several large petioles as probably belonging to arborescent species, and also two trunks covered with aërial roots, viz. *Psaronius erianus* and *P. textilis*. He also referred to *Caulopteris Peachii* of Salter as the only tree-fern known in the Devonian of Europe. He then described remains of four species of tree-ferns in collections communicated to him by Dr. Newberry of New York. The first of these, *Caulopteris Lockwoodi*, was found by the Rev. Mr. Lockwood at Gilboa, the locality of the Psaronites already mentioned, in rocks of the Chemung group. It is a fragment of a well-characterised stem, with parts of five petioles attached to it, and associated with remains of the leaves. It must have been entombed in an erect position, and is not improbably the upper part of one of the species of *Psaronius* from the same locality. The second species, *Caulopteris antiqua*, Newberry, is of much larger size, but less perfectly preserved. It is a flattened stem on a slab of marine limestone from the Corniferous formation in the lower part of the Middle Devonian (Erian) of Ohio. The third species. *Protopteris peregrina*, Newberry, is from the same formation with the last, and constitutes the first instance of the occurrence of the genus to which it belongs, below the Carboniferous. The specimens show the form and arrangement of the leaf-scars, the microscopic structure of the petioles, and also the arrangement of the aërial roots covering the lower part of the stem. The fourth species is a gigantic *Rhachiopteris*, or leaf-stalk, evidently belonging to a species quite distinct from either of the above and showing its minute structure. It is no less than four inches wide at the base. In the cellular tissue of this petiole are rounded grains similar to those regarded by Corda and Carruthers, in Carboniferous and Eocene specimens, as starch-granules. In addition to these species, the paper described a new *Nœggerathia* (*N. gilboensis*), and noticed a remarkable specimen from Caithness, in the collection of Prof. Wyville Thomson, throwing light on the problematical *Lycopodites Vanuxemii* of America; also interesting specimens of *Psilophyton* and other genera seen by the writer in the collection of Mr. Peach of Edinburgh. Dr. Duncan doubted the desirability of basing generic and specific terms on imperfectly preserved and indistinct specimens, and pointed out the disagreements among botanists that had resulted from so doing. He would prefer calling fossils such as those described "cryptogamous forms from certain strata." He was doubtful also whether the supposed petrified starch was not merely orbicular silex. The chairman remarked on the four different conditions exhibited by existing tree ferns, first, with roots running down the stem: secondly, the lower portion with oval scars; these are, thirdly, farther up the stem, rhomboidal vertically; and, fourthly, higher up still, rhomboidal horizontally; so that were the plant fossil, distinct genera and species might be founded upon the different parts.

MANCHESTER

Literary and Philosophical Society, March 21.—Mr. E. W. Binney, president, in the chair. Dr. John Hopkinson was elected an ordinary member of the society. "On the Mechanical Equivalence of Heat," by the Rev. H. Highton. The following is an abstract of the arguments as given in the paper and brought out in the subsequent discussion:—1. The author apologised for having mentioned other names in connection with great discoveries which were undoubtedly due primarily to Dr. Joule, and spoke of the very great value of Dr.

Joule's experiments, even when he did not agree with the deductions drawn from them. 2. The subject is of extreme importance both for the interpretation of physical phenomena and or determining what limits are assigned by the stern laws of nature to the exercise of man's mechanical and scientific skill. 3. No doubt Dr. Joule has ascertained the heat ordinarily derived from the destruction of energy, by means of friction with various substances; but it has been assumed, *in defiance of facts*, that the numerical relations which connect heat and energy in the case of friction hold good when energy and heat produce or destroy each other by any other means. 4. In the case of friction itself, energy is not transformed simply into heat, but partly into heat and partly into another kind of energy, which is involved in the expansion of the solids or liquids acted on. 5. No doubt the coincidence between the mechanical equivalent of heat, found by Dr. Joule from friction, and that by M. Favre from working a magnetic engine, seems very striking; but (1) the value of Favre's experiment disappears on examination. It was but a single experiment, either never repeated, or never repeated with the same results; in a very delicate experiment there was only the difference of 300 units out of 18,000; and even the permanent enlargement which always takes place in magnets which are in use might account for this; and (2) numerous and long-continued experiments by M. Soret show results entirely discordant with the single one of M. Favre. 6. It seems incredible, that with the imperfectly constructed engine used by Joule and Scoresby, they should at the very first trial have succeeded in utilising two-thirds of the magnetism evolved, or capable of being evolved, by their battery; and Dr. Joule now tells us that according to his latest calculations of the mechanical equivalence of heat they utilised six-sevenths of the power of the battery. The only conclusion we can arrive at is, that the real power of the battery, and therefore of a grain of zinc, must have been much greater than he calculated. 7. For consider the disadvantages under which the engine acted: (1) the temporary and permanent magnets were never nearer than $\frac{1}{4}$ of an inch apart. Though Dr. Joule assures us this does not affect the *power* of the engine, it certainly produces a waste of zinc, as the near approach of the magnets creates counter-currents which check materially the consumption of zinc. (2) The copper wire was not tested for conductivity; a subject little thought of at that time, and it is found that a very small impurity in copper wire will very, very largely diminish the power of an electro-magnet. (3) The iron was not tested for specific capacity for magnetism, yet this is a most important point which is even now but little appreciated. It is found practically that, if two electro-magnets be made from the very same piece of iron, most carefully prepared, with the very same length of the same wire, without the slightest assignable cause, one will sometimes have three times the power of the other. Hence I conclude that the maximum energy capable of being evolved by a grain of zinc must be very much greater than that assigned to it by Dr. Joule. 7. Dr. Hopkinson's argument, in his paper lately read to this society, virtually amounted to this—that a well-constructed magnetic engine will get no more duty from a grain of zinc than an ill-constructed one; and consequently, I presume, that magnets might be weakened to any extent, and removed to ever so great a distance from one another, without necessarily affecting the efficiency of the engine. 8. Dr. Hopkinson has in his criticism strangely substituted $(a-b)$ for (b). In Joule and Scoresby's paper, the consumption of zinc is expressed not by $(a-b)$ but by (b); and consequently the duty of a grain of zinc not by $\frac{W}{a-b}$ but by $\frac{W}{b}$; and when the magnets are stronger and approach nearer to each other, even if W be not increased, (b) is diminished. 9. My argument was this, that since the accepted theory of the mechanical equivalence of heat is *that production of energy absorbs, and destruction of energy produces, a definite amount of heat*, if we find cases, as those of elastic wires, and water below its maximum density, in which destruction of energy produces cold, not heat, then the doctrine of the mechanical equivalence of heat cannot be true; we might with equal justice call it a mechanical equivalence of cold. It is no reply to say that such facts are simple deductions from the laws of thermodynamics. This would only show that the laws of thermodynamics are inconsistent with the doctrine of the mechanical equivalence of heat. 10. The argument from the fire syringe I withdraw, as inconclusive. But I think my case was sufficiently established without it. 11. Joule and Scoresby in their paper incorrectly assume that if the quantities of electricity in the current at different times be represented by (a) and (b), the heat varies as a^2 to b^2. This is only true where the resistance is the same. In the case before us the working of the engine introduces a fresh element in resistance. 12. Again by assuming that $(a-b)$ represents diminution of quantity of the current, *and* the diminution in the zinc consumed, *and* the heat converted into useful work, they involve the supposition either that less zinc produced equal heat, or that heat was changed into useful work which was never produced at all, and therefore could not be absorbed. In fact, there was no *proof* that any heat was absorbed at all. 13. It is said that in electro-plating, electro-magnetic engines, worked by steam, are found more economical than batteries. This is in cases where a battery of many cells would be required; which is always wasteful, as a large number of equivalents of zinc must be consumed to deposit one equivalent of silver or other metal. 14. Besides, there is a far greater advantage in changing work into electricity, than electricity into work. In the former case all, or nearly all, the work is effective; in the latter, a very small portion of the electricity has hitherto been utilised.

—Dr. Hopkinson said that most of Mr. Highton's objections to the mechanical equivalent of heat appear to arise from a mistake as to what is meant by the term. The nature of this mistake may be best seen in the case of a perfect heat engine, of which t_1 and t_0 are the absolute temperatures of the source and refrigerator. Then from every unit of heat leaving the source we obtain $\frac{t_1-t_0}{t_1}$ J units of work. Now this a quantity variable with t_1 and t_0; it would be similar to most of Mr. Highton's arguments to infer that from a given quantity of heat a variable quantity of work could be obtained. But, of course, the case really is that of the unit of heat leaving the source, $\frac{t_0}{t_1}$ is lost in the refrigerator, whilst $\frac{t_1-t_0}{t_1}$ disappears *as heat* and is converted into the work done, and the principle of the equivalence of heat and work asserts that J is constant. It will be seen that this is the mistake Mr. Highton makes in his paper in the *Journal of Science* (end of article 6). He seems there to imagine it stated, that the work done is equivalent to the whole heat thrown into the gas, and he fails to perceive that a certain portion is used to raise the temperature of the air or turpentine. This will make my criticism of his paper in the *Chemical News* clearer. Mr. Highton argued against the mechanical equivalent, and what I pointed out was, that the chemical energy, which was converted into mechanical effect and not used to heat the wire, was proportional to $a-b$, that therefore, in order to prove that there was no mechanical equivalent Mr. Highton must show $\frac{W}{a-b}$ is variable. I do not assert that a badly constructed engine will get as much heat from fuel as a good one, but merely that the work done and the heat, which has disappeared as heat and been converted into work, are in a constant ratio. Now as regards Mr. Highton's argument from the case of elastic wires—that the wire will be cooled when stretched follows from the two laws of thermodynamics, a proof may be seen in Tait's Thermodynamics, p. 105. Mr. Highton replies, "Quite true; but this only shows that one of the laws of thermodynamics is inconsistent with the doctrine of the mechanical equivalence of heat." Now the first law of thermodynamics asserts nothing else than that there is a mechanical equivalent, constant in all cases; whilst the second law, as usually stated, involves the first law, and involves nothing else but Carnot's axiom and the principle that in conduction heat flows from the hot to the cold body, both of which no one will doubt. Mr. Highton's reply is very similar to stating that one of Kepler's laws is inconsistent with the planets moving in ellipses. What Mr. Highton proposes as a paradox is then a necessary consequence of the principle he attacks. Though the doctrine of the mechanical equivalent of heat finds its firmest basis in the immortal experiments of Dr. Joule, the fact, that assuming it we can explain many phenomena, is a valuable supplementary proof.

EDINBURGH

Royal Physical Society, February 22.—Dr. Robert Brown, President, in the chair. "On the Glacial Epoch," by the Rev. P. A. Brodie. The author of this paper proposed three questions—(1) Is he correct in supposing that the popularly received opinion with respect to the glacial epoch regards it as a period of com-

paratively limited duration, intermediate between the Tertiary and Quaternary eras, when a freezing climate prevailed contemporaneously over a great part of the globe? (2) Has he stated with sufficient distinctness the facts adduced in support of that opinion? (3) Do the arguments which he has brought forward prove the opinion to be erroneous?—"Notes on the Sea Otter (*Enhydra marina*, Flem.)," by Pym Nevins Compton.—"On the Tailless Trout of Islay," by Colin Hay and Peter M'Kenzie. Mr. Peach, before reading the communication he had been entrusted with, wished to say that a gentleman whom he met at the house of a mutual friend, mentioned the Tailless Trout of Islay, and as this excited Mr. Peach's curiosity, the gentleman procured from Messrs. Hay and M'Kenzie the bounteous supply now laid before you. The communication was kindly made in reply to questions put to them by Mr. Peach:—"The locality of the loch is about 1,000 feet above the level of the sea, and is on the estate of Mr. Finley of Elenlossit, Islay, and at its highest water is not more than an acre in extent. It is so shallow that a man could wade all through it; the bottom loose stone quartz, same as the surrounding mountains, and we think it is the most elevated piece of water on the island in which trout exist. It is named Lochna Maorichean, meaning that a species of fresh water 'limpet' or 'whelk,' is found on its shores, but we can say nothing about these, all we have gone there for was to capture some of its strange denizens; as to its other productions, there are small tracts of weeds here and there lying on the surface of the water, with soft pulpy stems; of the parasites of the fish or in the water we cannot speak.—Dr. J. A. Smith exhibited a specimen of the *Cottus Grœnlandicus* (the Greenland Bull-head), recently taken at the Firth of Forth.—Mr. David Grieve exhibited a photograph of the Queensland Cicada, or as it is popularly termed, Locust.

MONTREAL

Natural History Society, Feb. 27.—The President, Principal Dawson, F.R.S., in the chair.—The President exhibited illustrations of new facts in Fossil Botany. The following is an abstract of his remarks:—"The first point mentioned was the occurrence in the Devonian Shales of Kettle Point, Lake Huron, of beds containing immense quantities of spore-cases, probably of *Lepidodendron*. These beds are referred by the Geological Survey to the horizon of the Genessee shales of New York, and are stated to be twelve or fourteen feet in thickness, and to extend over a considerable area of country. Specimens in the collection of the Survey show that the bituminous matter which causes the combustible quality of the shale, is due entirely to the immense quantities of spore-cases present, which, under the microscope, appear as flattened discs scarcely more than one hundredth of an inch in diameter. Specimens of the trunks of *Lepidodendron Veltheimainum* and *Calamites inornatus* occur in the same beds. This is probably the oldest bed of fossil spore-cases known; but in later geological periods similar beds occur, the Tasmanite, or 'white coal' of Tasmania, which consists of spore-cases of ferns, being a notable instance. The author next referred to the discovery of specimens indicating the existence of three or four species of Tree-Ferns in the Devonian of New York and Ohio. He had received from Prof. Newberry of New York a specimen, showing the upper part of a stem with five leaf stalks attached to it. This he had named *Caulopteris Lockwoodi*. Three other specimens collected by Prof. Newberry in Ohio indicated the existence of three distinct species belonging to two genera. The two most important had been named by Prof. Newberry *Caulopteris antiqua* and *Protopteris peregrina*. They are from the Coniferous Limestone, and thus carry down tree-ferns to the bottom of the middle Devonian. One of them has the cellular structure and vascular bundles in such preservation as to show their microscopic structure, which is precisely similar to that of modern ferns."—Mr. A. R. C. Selwyn, Director of the Geological Survey of Canada, read a paper "On the Occurrence of Diamonds in New South Wales," by Mr. Norman Taylor, late of the Geological Survey of Victoria, and Professor Thompson, of the University of Sydney.

PARIS

Academy of Sciences, March 27.—The hall was pretty well filled, and the correspondence was rather heavy. Letters from the provinces and from foreign parts were numerous, as the insurgents had not taken possession of the Post Office, and communications were not stopped between Paris and the outer world. M. Faye presided over the sitting, which was as orderly as in former times. No trace of public emotion was to be seen in the hall where the scientific assembly meet. Numerous details were given of the meteor which was seen on the 17th inst. in southern France, and left behind an immense luminous track. These details were very welcome, as during the investment certain bold theorists maintained that falling stars, bolides, and meteorites, were produced by the same causes. New facts having been brought forward at the last sitting to show that atmospheric changes are produced in high altitudes and gradually manifest themselves in the vicinity of the air, M. Wilfred de Fonvielle sent a communication upon the truth and genuineness of this observation. He quoted letters he had received from M. Buys Ballot, the learned director of the observatory at Utrecht, when he was waiting for a favourable wind in order to return to Paris by an aërial expedition during the investment. And he concluded by showing that the best way for ascertaining the state of things at a high level was to try scientific ascents. The Academy appeared to be much pleased with the idea, but it is impossible for it to recommend the application of the scheme as long as order is not established in Paris. M. Delaunay and M. Sainte-Claire Deville disputed as to the meaning of the thermometric measures which had been taken during the investment of Paris at the observatory at Montsouris and in the Jardin des Plantes. The distance of the two stations is something less than a mile, and the difference in altitude is about thirty feet. This circumstance may account for the difference in the two sets of observation.

DIARY

THURSDAY, APRIL 6.

LINNEAN SOCIETY, at 8.—On the stigmas of *Proteaceæ*: G. Bentham, Pres. L. S.—On the generic nomenclature of *Lepidoptera*: G. R. Crotch.

CHEMICAL SOCIETY, at 8.—On Burnt Iron and Burnt Steel: W. Mattieu Williams.—On the formation of Sulpho Acids: Henry E. Armstrong.

SATURDAY, APRIL 8.

ROYAL SCHOOL OF MINES, at 8.—Geology: Dr. Cobbold.

TUESDAY, APRIL 11.

PHOTOGRAPHIC SOCIETY, at 8.

WEDNESDAY, APRIL 12.

SOCIETY OF ARTS, at 8.—On Boiled Oil and Varnishes: C. W. Vincent.

THURSDAY, APRIL 13.

MATHEMATICAL SOCIETY, at 8.

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NOTICE

We beg leave to state that we decline to return rejected communications; and to this rule we can make no exception.